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Eugene P. Visco Interview (MORS)

Visco, Eugene P.

Military Operations Research Society Oral History Project Interview of Eugene P. Visco, FS

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This article is extracted from a MORS Oral History Project interview of Eugene P. Visco, FS. Another portion of the interview has been published as "One Analyst's Experience with Chemical Munitions Testing."

MR. SHELDON: We're here today on behalf of MORS to interview Mr. Gene Visco. Today is the 30th of October 2000. We're conducting this interview in Carlisle, Pennsylvania, because MORS is holding a C4ISR workshop at the Army War College this week. My name is Bob Sheldon. Participating with me in this interview of Mr. Visco is Jack Marriott, currently Chair of the MORS Heritage Committee. Gene, I would like to start out by having you give your name, rank and serial number, and any other basic information you'd care to provide.

MR. VISCO: My full name is Eugene P. Visco, but I'm known as Gene most of the time. I was born in 1927. My e-mail address is gvisco@bellatlantic.net. I'm retired, doing some part-time work for a little company called Simulation Technologies, Incorporated. Despite my cynical attitude toward models, I'm doing verification and validation of a Marine Corps chemical/biological model. I'm doing some independent research, principally on an analytic process for operations other than war. I'm still trying to work on friendly fire and on human behavior in combat. I teach a course in the history of operations other than war at George Mason University. I'm trying to get my students to develop analytic tools on operations other than war.

MR. SHELDON: What was your job three years ago, when you retired from the Army?

MR. VISCO: In Federal Government terms, I was a supervisory operations research analyst. I was in the Office of the Deputy Under Secretary of the Army for Operations Research, Walter Hollis, FS, who is the Army Sponsor of MORS, and Jack Marriott was in the office toward the end of my tour. I worked for Walt for ten years. Prior to working for him, I worked for the Chief of Staff in a similar capacity.

MR. SHELDON: Where were you born, raised, and educated?

MR. VISCO: I was born in Boston, Massachusetts. People tell me that still creeps into my dialect. I lived in Boston until I was about 12.

I went to public school in Boston and actually started high school there at what was then called Mechanic Arts High School, which was an engineering high school. It taught mechanical drawing, drafting, engineering skills, preliminary kinds of things.

I went for my freshman year there, the Depression was just ending, with industries getting ready for the war. My father got a job in a foundry in Worcester, Massachusetts, which was a big industrial town in the central part of the state.

We moved to a little town called Shrewsbury, just east of Worcester, and I continued high school there in my sophomore year. That high school had a college prep program and a commercial program for kids who weren't going to go college. I learned typing, a little bookkeeping, and that sort of thing.

I started there in the fall of '41, and we know what happened that December. So very quickly, I switched from the commerce part of the program to a college program. People were thinking about what things were going to be like after the war, and the high school revamped its program so that it made an enormous number of mathematics and science courses available. It also included an aeronautics course which all the male students took.

I spent the last two years of high school taking lots of mathematics and lots of science. In April of '44 when I turned 17, I enlisted in the Navy. The Navy allowed me to complete my high school work. I graduated two months later and reported for active duty about two or three days after graduation in June.

I went into a naval aviation program but as a sailor, not as an officer candidate, and I was enlisted as something called air crewman. I was trained as an ordnanceman, and I was given a modest amount of training in radar operation, which was very primitive at the time. All the air crewmen had to go through that.

Then I went to gunnery school. There was one story about my earlier experiences in the Navy, when I was going through aerial gunnery school. During the war, the Navy taught aerial gunnery, starting out by using shotguns and shooting at clay pigeons to give us some training in how to lead the clay pigeon as though you might be leading at attacking aircraft, if you were flying an airplane with hydraulic turrets, and gunnery. Using shotguns was easier and cheaper than using a machine gun.

We had ring sights on the shotguns, very much like the ring sights that we would have with our machine guns in the turrets that are on the airplanes. But there was one little significant test that the instructors had us go through. We had to determine what was called the master eye,

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and the master eye is the dominant eye. One of your eyes, normally, is a dominant focusing or aiming eye. The little test that you use to determine your master eye is you hold a finger out at, let's say, the intersection of the walls in a room with the ceiling. Now you've got to point. You do that with your eyes open, and then you alternately shut one eye and find out whether the apparent location of your finger moves.

It turned out that when I did that, my left eye was the master eye. The training procedure said you should shoot from the left shoulder, if your left eye is a master eye for aiming purposes.

I, being right-handed, didn't want to do that, even though I found out I was left eyed. So I began shooting from my right shoulder and I couldn't hit any of the birds. The instructor looking over my shoulder was commenting on the fact that if I didn't start hitting some birds, I was going to be washed out of the program.

Then he asked me which was my master eye, and I said, grudgingly, that my left eye was the master eye, and he chewed me out for not shooting from the left shoulder.

So I shifted to the left shoulder. It was very awkward to do that, because being right-handed, it's hard to start using your left hand against your left shoulder. But it was rather amazing how important that dominant eye was, because I began hitting the birds and bringing down very credible numbers of birds, and showed that the Navy was right in demanding that I do that.

After gunnery school, I went to something called operational training where I linked up with a crew. I was assigned to a patrol bomber, a Martin Mariner, built by Martin, long-range patrol bomber, which stayed aloft 15–18 hours without refueling.

MR. SHELDON: What kind of patrol bomber?

MR. VISCO: Long-range patrol. It was a Martin Mariner, twin-engine patrol bomber with enormous fuel tanks and very small engines. I trained with a crew, three officers and nine enlisted on the airplane, three mechanics, three radiomen, and three ordnancemen. I was a bow gunner who operated the bow turret.

We trained in Corpus Christi, Texas. We went out to San Diego and trained some more, then went up to San Francisco and trained some more. By this time, the war was over. The war ended in August of '45, and we were sent on out to help bring all the airplanes back from the Pacific. We flew from Alameda—we took a brand new airplane out of Alameda Naval Air Station, near San Francisco, to Kaneohe Bay, Oahu, which at that time was a naval air station.

It took us almost 18 hours to fly from San Francisco to Hawaii, but we had no navigation problems. We hit the island right on the button. It's just a slow airplane.

At that point, they didn't need ordnancemen and gunners anymore, so they dropped me off the crew and I stayed at Kaneohe Bay for a while. In the Navy, when you end up as a supernumerary, either on a ship or on a shore base, you're put to work. So I was assigned to something called beaching crew—Mariners were not amphibious. They were flying boats, and when they came in, they landed in the water, then taxied up and hooked up to a buoy in the bay. The buoy had a line running to a cement block in the bottom of the bay, and then through a ring, or cleat, on up the shore.

When the airplane was hooked to the buoy, it would cut its engines and just drift, but it was tied up to the buoy. A small boat would come out from shore with another line and hook on to a tail hook—these airplanes all had a ring on the tail, and two sailors would swim out to the airplane towing what was called beaching gear, which was just a big wheel with a floatation box on it, which they would float, and you hooked this on to the side of the airplane with the help of somebody inside the airplane.

My job was to swim out to the airplane, towing one of these beaching gear things, and then when you have the gear on, you would simply tow the airplane up on the beach, feeding the line out from the bow—it was hooked up through the buoy—and towing it up with the tail end and then towing it right up a ramp on to the beach.

Needless to say, I was very healthy and bronzed, and my job was just to lie there in a pair of trunks, all day long, and wait for airplanes to come in. Only a few planes a day would come in. They'd be flying back from further out in the Pacific. Eventually, they did decide they needed me in a squadron, so they shipped me out to the Philippines. I joined a squadron, VPB 28, which was stationed at Palawan, the southwestern-most island of the group. It's a long, thin island, about 500 miles long and about 20 miles wide. Puerto Princesa was the one major town on the island, and the squadron was stationed there. Later, the squad-

ron moved up to Cavite which is on Luzon. Cavite is on Manila Bay.

I came home when my points came up. We calculated points for returning home based on how much time in service, and how much time overseas, and if you'd been in campaigns. So eventually I came home. I toyed with the idea of staying in the Navy but I decided to come home, go to college, and then see what else I wanted to do after that.

MR. SHELDON: How much total time did you have in the Navy?

MR. VISCO: I only had a couple of years, most of it in training because of the technical training they put me through, well over a year of training. When I went to ordnance school, not only did I learn all the weapons that the Navy had, all weapons that were on aircraft, but also all hand weapons including rifles, including the BAR (Browning Automatic Rifle). I knew how to strip a BAR, blindfolded. It dates back to World War I. It was a hell of a good infantry weapon. A big heavy thing.

MR. SHELDON: Before we leave your Navy career, anything you learned from your Navy operations that helped you as an operations analyst for the Army?

MR. VISCO: I suppose a little bit. Some notion of military customs, a little bit about military operations. One of the things that we were taught in ordnance school is how to make our own tools. We actually made open-end wrenches. We also learned about bombs and bomb racks, and air-delivered torpedoes, and mines. It took a long time to teach us all that stuff.

MR. MARRIOTT: You were the gunnery—

MR. VISCO: Yes. I was. The ordnanceman is the guy who maintains weapons. The Navy calls them ordnancemen. In my case, it was aviation ordnanceman. I eventually got to be a 3rd class petty officer, equivalent to an old "buck sergeant." It's the first of the noncommissioned officer ranks.

I liked it out in the Philippines and I really toyed with the idea of signing up. During the war, they had what they called minority enlistments. Minority enlistments were—you were enlisted until age 21, or the war plus six months, whichever came later. They tried to recruit a lot of people to stay, but I decided to go home, go to school.

My folks had migrated to Florida from Massachusetts. My father had worked in a foundry all through the war on the night shift. Terrible job. He was frozen in the job like most people were in the war industries. So the day the war ended, he quit his job. They packed up the household goods and drove down to Florida. When I got discharged, I went down there to live with them. The nearest university was University of Miami in Coral Gables. I enrolled there in the fall of '46 and did an undergraduate degree, mathematics and physics.

MR. SHELDON: How'd you happen to choose math and physics?

MR. VISCO: I was doing pretty well in math in high school. When I went to gunnery school in the Navy, there was a certain amount of mathematics involved there—how you calculate how to lead an enemy aircraft with your guns. We didn't have computing gun sights. So you had to learn a little bit about the trigonometry. As an aircraft is approaching you from the front, it's vastly different from an aircraft approaching you from the rear, and how you lead. I had this affinity for mathematics anyway. I did pretty well with the computational part of mathematics.

When I graduated in 1950, the country was in a mild recession following the big productivity surge right after the war. I applied to a lot of places but I couldn't get a job.

I pumped gas for a while, and I was working in the summertime in the Hollywood, Florida volunteer recreation program. I'd gotten interested in theater. I'd done some acting in college. So I did a little program with kids in the summertime, teaching them a little bit about theater, makeup, and how to stage a play. We actually put on a couple of one-act plays!

Hollywood was the location of a military school called Riverside Military Academy, which had its main campus in Gainesville, Georgia. It's a small town in the mountains of Georgia.

The old general ran a very expensive military high school and prep school. He would have his kids report in the fall in Gainesville, Georgia. Then at Christmas break, he would move to Hollywood, Florida for the midwinter term so the kids could be close to their rich parents vacationing in Miami Beach or in Hollywood.

Then in the spring, they'd close up and move back to Gainesville, Georgia. He was looking for a drama coach, and he asked somebody in Hollywood did they have any ideas, and they said, well, there's a young guy who's

working with these kids. This old general offered me a job to teach physics and public speaking—a crazy mixture, and to be the drama coach too! He really needed a drama coach. I jumped at it. I reported in September of '50 to Gainesville, and was introduced as a junior member of the faculty. Had to wear uniforms, most of them wore Army type stuff. I think my pay was $1200 a year, but I got room and board, such as it was.

I discovered very quickly that that was not for me. You needed to be a real disciplinarian. There were some kids there who were second and third generation Riverside kids, whose fathers, grandfathers, had gone there and gone on to military careers. It was good prep school for getting kids ready for the military. But there was another bunch, there because their parents put them in a military school because they couldn't handle them otherwise. I was about 21 years old. Some of these kids were bruisers; you didn't want to discipline them!

I really wasn't terribly well prepared to be a teacher. When I got the job, I went back to University of Miami to take some teaching courses. But it didn't teach me very much about lesson plans, and the kind of things that you really need. So I muddled along. The dean would evaluate me periodically, and sadly shake his head.

When we got ready for the end of the fall semester and packed up the school to move it, I had to pack up my physics lab. When we got down to Florida just before Christmas, I had a week off.

The big event of the summer of 1950 was the Korean War. I was in the Navy Reserves but I'd finished up a Reserve tour. I got a nice old-fashioned telegram which said would you like a job working for the Army. They offered me a GS-5 to work at Dugway Proving Ground in Utah. I didn't have a clue of what Dugway Proving Ground, Utah, was, but the GS-5 salary was around $3200 a year, which was considerably more than I was making teaching.

I found out that it was a chemical facility. After five years at Dugway, I decided I really wanted to do some graduate work. I had met some people from the Johns Hopkins University Operations Research Office (ORO), which had been established in 1948 to provide an operational research capability to the Army. I had met a number of the ORO people who had come out to Dugway, because they were beginning to do a major chemical study for the Army. I was very much impressed with them.

After a set of interviews, I got a letter from the director, Ellis Johnson, the only director that ORO ever had. He invited me to join the staff. I came back to Washington in the fall of '56. That's when I got the title of operations analyst. There were about a 100 analysts working at ORO. All different kinds of folks: mathematicians, physicists, engineers, biologists, agronomists, psychologists, a couple of MDs. Quite a diversified staff, the mixed team concept, to the ninth degree. But everybody was called operations analyst.

MR. SHELDON: Where were their offices located at the time?

MR. VISCO: At that time, the main office was located at 7100 Connecticut Avenue in Chevy Chase, Maryland. Prior to World War II, it was a girls finishing school, a very elegant school, and apparently it went broke, was sold for taxes. During the war, the 4-H Club took it over. After the war, building materials were in scarce supply and the place needed a lot of work. The Army was looking for a place to put ORO.

ORO was first located at Fort McNair, the administrative offices. When it first started, it was called the General Research Office for a few months, and then took on the name, Operations Research Office. Ellis Johnson had his office at Fort McNair, but they knew that the place was temporary.

So the Army went to the 4-H people and they said, look, let us rent this place from you for a few years, we'll guarantee that we'll improve it somewhat, we'll put in some money to fix the floors so the safes won't drop through from the second floor to the ground floor. You'll get it back in no worse shape than it's in now and maybe a little better shape, and you'll have the money from the rent, and then building supplies will be more readily available. The 4-H agreed. So ORO was located in what used to be a girls finishing school.

They put me on a chemical study, a big study for ORO at the time, which had about eight or nine analysts. We also had a handful of soldiers that the Army assigned, professionally educated soldiers, to help out.

We were located at the Science Building. The outer office in the Science Building where the secretaries for our project were located had a beautiful crystal chandelier. It had been the Domestic Science Building where the young

ladies learned to set a formal dining table under this beautiful crystal chandelier.

As the staff expanded during the Korean War, we took over other rental sites in Bethesda and Chevy Chase, scattered all around. We had some chauffeured sedans that would run us around and do messenger service. We had a vacuum tube computer, 1103A Univac. It was located in a very large Quonset hut-shaped building, just a huge sheet metal building located near the railroad tracks in Bethesda, Maryland. Trains were still running in those days, near what is now called the Air Rights Building in Bethesda. This enormous vacuum tube computer had to be shut down every summer because we couldn't cool the building well enough with the window air conditioners.

Ellis Johnson decreed that all the analysts had to learn how to program. So we did the punch card business and the paper tape stuff. Your final exam was to write a program to compute the mean of five two-digit numbers. If you could do that, you passed the course! The vacuum tube thing was our computer for quite a long time.

ORO also pioneered the train fire targeting system. Traditionally, soldiers were trained on what was known as a fixed-distance range. Go out and shoot at targets. Very boring for soldiers, and very impractical. It's got nothing to do with military operations. ORO came up with the idea of using pop-up targets, randomly distributed around a field. They developed a concept called train fire, which the Army still uses to train its soldiers. They have these random things popping up. They used to be called cocky Kens, because one of the key guys doing the study was Ken Yudowitch, who had a fantastic reputation as a womanizer.

We also had an engineering lab. In it were down-to-earth, hard-working engineer types who built things. They could put together electronic gear. For example, on this pop-up target thing, one of the things you wanted was some way to determine which target was being hit, kind of a precursor of the system we use out at the National Training System now. The NTC system is where you fire a laser at a target and the target lights up when it gets hit. Well, this was a precursor of that system.

A lot of that stuff was built in the ORO laboratory. The guys would build things to help out the analysts who are going to go out in the field and do an experiment.

One summer, a bunch of analysts went out to Gettysburg, and there was a big debate as to what Pickett's charge really must have looked like. So they decided that they would start at that far end, and see how far they could run across that field. It turned out to be a major undertaking because it's got an uphill slope. It was a hot July day when Pickett and a couple of divisions made that wild charge. ORO used to do things like that, just to get a feel for what the operation's really like.

In those days, ORO was really close to the Army and got out in the field. During the Korean War, which was before my time because I didn't get there till '56, about half the analysts at ORO spent time in Korea working in the field, doing all kinds of different studies. There was that long tradition of working closely with the field forces and the ground forces, to get a good understanding of Army problems. We've lost that in operations research now.

After the chemical study in 1961, I began working on a very early version of a cost-effectiveness study, a study that was pioneering for its time, trying to relate cost and effectiveness of big systems in the Army. We took on the future main battle tank, which later became the M1. I worked for Eddie Cushen, and we tried to develop some mechanism for doing this kind of study, because nobody had ever done it before.

MR. SHELDON: You're still at ORO doing this?

MR. VISCO: ORO, still. We decided to use an early simulation, an early model called Carmonette, which later became a much bigger model. At that time it was a small model with half a dozen tanks, some antitank weapons, and such.

That was the way we were going to do the effectiveness side. Then we were going to build a cost model, to try to build a tank component by component.

We were thinking of combined rocket launcher and internal projectile like the Shillelagh. We were thinking of diesel engines. We built it up by looking at cost data on existing systems and trying to do some projection.

The study went on for a couple years, and ORO went out of business, some complications with the Army. The ORO ceased to exist on the 31st of August 1961, and the Research Analysis Corporation (RAC) came into being on the first of September 1961. RAC was an Army not-for-profit, free-standing institution. Johns Hopkins withdrew from the process. It was a political

disagreement between the Army and the leadership of ORO.

I stayed on; a few people resigned in protest, and went on doing other things. Eddie Cushen was one of those who resigned and went with Ellis Johnson out to Case Institute to do other research, and so I took over the study.

We made runs with Carmonette, did some effectiveness measures, matching our tanks against Russian tanks, ran a whole series of runs, did our costing. Not a bad study when it was finished. But the thing that bothered us when we got all through with this calculation was, we could not believe that anyone in his right mind would pay that much for a tank.

The M48 tank was the standard tank in the Army at the time, and the M60 was just coming in. The M60 cost about $100,000. This new tank was going to cost three-quarters of a million, and we couldn't believe that anybody in his right mind would pay that much for a tank. When we wrote the report, we emphasized the effectiveness side. We put the cost thing way in the back somewhere, because we thought it was an asinine number.

If you take 1962 data, and you come forward to present day, and you look at the price of the M1, and you do the usual discounting, that three-quarters of a million is about what the M1 is costing, which is now a couple a million dollars, or more.

RAC opened a field office in Germany in 1962, and I got a chance to volunteer for that. We had a team of five analysts, headquartered in Stuttgart-Waingen, a suburb of Stuttgart in southern Germany. We were on a post that the U.S. Army calls Patch Barracks, named after General Patch from World War II. We were attached to the Army G-3, five analysts and a secretary. There were two senior and two middle grade analysts, and one youngster on the team. At that time U.S. Seventh Army consisted of five divisions in two corps: VII Corps and V Corps. V Corps was located just outside of Stuttgart; VII Corps was headquartered in Frankfurt.

We also had two armored cavalry regiments with Seventh Army, so it was a real field army. We also had the III German Corps attached to the U.S. Seventh Army, so it was a three corps army. A lot of soldiers scattered all over southern Germany. The Brits had northern Germany.

The first thing we did was to go out to visit all the divisions and cav regiments, talking to the senior staff, principally the G-2 and the commander, if we could get to him. We wanted to find out what kind of problems they saw themselves having in their sectors, and their role in NATO. Were there things that we might be able to help them with? We didn't have a work program. We were building one by talking to the units.

We gathered these different ideas, came back in, and developed an analytic structure. What we tried to do was learn what these guys saw as their problems.

Secondly, we asked ourselves was it something we could work on, given there were just the five of us with different capabilities? We could call on some help from home, but we felt we ought to be able to do most of it ourselves. Not only could we do it because we had the smarts—but could we do it in a relatively short period of time?

So we looked at all of the different ideas and put them through this mill, and we came up with a priority listing. The top item on the list was the role of helicopters in the event of a major war in Europe. I forgot to preface this. In the first year of the Research Analysis Corporation's existence—from the fall of '61 to the summer of '62, the Army had begun a major study of the role of rotary wing aircraft in future war. The study had been dictated by Secretary of Defense McNamara. The Army was ordered to do it. The study leader was Hamilton Howze, the commanding general of the XVIII Airborne Corps at that time. Therefore the study was headquartered at Fort Bragg, headquarters of the corps. But the study was being done by literally hundreds, if not thousands of people. RAC was doing the study; RAND was doing pieces of it. SRI was doing pieces of it. The Army was doing a lot of pieces of it.

They were doing experiments. They were doing trials. They were doing demonstrations. STAG (Studies Tactics and Analysis Group), the precursor of CAA, STAG was a war-gaming group, out in Bethesda, was working on it. Everybody was working on it. It was like what they're doing now with transformation studies. A lot of power was being vested in this.

It was the winter/spring of '62. I was told to do a study on interdiction of supply lines in Southeast Asia from aircraft. This is before the war of course. The question was what could be done to interdict supply lines?

I was using the Russian IL-28 bomber, a light bomber, and I was using World War II calculations of the hit-kill probabilities from iron bombs on railroad tracks and bridges. It's very hard to destroy bridges and railroad tracks with iron bombs. It's very, very difficult.

I wrote a paper on this, and then I was told to take it to Fort Bragg—this is early summer of '62—and brief the Howze board. The board was a very prestigious group. They would listen to all this stuff and do some kind of synthesis. I'm assuming I'm going down to Fort Bragg in the morning and coming back in the afternoon. I just take an overnight bag. The board is working out of the high school in Fort Bragg, which is the only air conditioned building in Bragg at the time. I report in, and find an old friend, Harold Linstone. Hal was working for Howze directly, and he was doing the synthesis of all of the stuff coming. And Hal says, "You know why you're here?"

I said, "Yes, I'm going to talk about this interdiction study." "No," he said, "you're here to work with me." So I had to go out and buy pants and shirts. I stayed for the summer working with Hal, trying to synthesize what was coming in from all these trials and these tests.

With that in mind, we return to Germany. The high priority study that the field Army people want is how our helicopters are going to be used. We put together a priority listing. We went across the street to meet the commanding general of Seventh Army, Bill Quinn. We get an audience with him, and our team leader Joe Bruner (very good analyst) gives him this briefing—how we did all this work program and what the priority projects are. And Gen. Quinn leans back in his chair and he said, "You guys can do whatever you want to do," he said, "but I know what I'm going to do with my choppers, and no study is going to change my mind."

So we saluted smartly and said, "Yes, sir." The study gets scratched off the list because if the commanding general doesn't want it, if he knows already what he's going to do with his stuff, then we're not about to do a study with our limited resources.

So we did a handful of logistics studies. There'd been a big debate in NATO about the adequacy of the pre-positioned equipment. The United States Army had put two division loads of armored equipment in France. France had not left the military side of the alliance yet. The idea was in the event of a war, we were going to fly soldiers over to marry up with their equipment and they would take the field.

The other NATO nations said that's crazy—it won't work. McNamara said let's show them that it'll work. So in the fall of 1963, after the Germans had harvested all their crops, because we were going to go across the fields, we ran the largest exercise that had ever been run in Germany, before and since, as far as I know.

We had two divisions in the field from the Seventh Army, one attacker and one defender. We ran the battle north and south so the Russians wouldn't get nervous about the exercise as they might if we ran it east and west.

We had an invading division coming out the north, a division from Seventh Army defending. We flew the 2nd Armored Division over from Fort Hood to France, married up with their equipment, marched through France up into Germany, passed through—I think it was the 8th Infantry that was doing the defending—passed through them and fought off the aggressor. An enormous exercise.

I was trying to build a simple hand-played war game for Central Army Group to use, principally for exercises, but also to help them think through operational plans. I wanted something a little more objective than the usual umpire stuff that we'd done for command post exercises.

I needed some factors. The VII Corps commander was going to be the exercise director on this big operation, and his name was Creighton Abrams, later commander in Vietnam, and later still, Chief of Staff; a good tanker from World War II.

So I went up to him, up in his Corps headquarters in the I.G. Farben Building, and I gave him a pitch. I said, "Sir, I'm trying to build this game, blah, blah, blah, and I'd like your permission to cover the exercise as an umpire but not play the role. But I'd like to be able to go back and forth on both sides." He said, "Sure; sounds great; do it."

It was very nice of him. So I spent the two weeks of the exercise with a jeep, and a driver, roaming the whole battlefield, making little notes on how fast units could move, and what they were shooting at. We didn't have any way of calculating casualties because we didn't have any of the fancy devices. It was great fun.

I was able to gather a lot of bits and pieces of statistics that I could stick into my game, to make it a little more realistic. I built the game using those factors as well as some other things.

I went around and talked to different units about their role.

A little sidelight. We had something called special ammunition supply points. These were people who handled the tactical nuclear weapons. When a war started, they were supposed to pick up from wherever they were and go to a pre-designed location, a location known to the field units. Because when they fired off the basic load weapons, they'd have to come back to the special ammunition supply point to be re-supplied. I wanted to know how long it would take the special ammunition supply point unit to move from its kasern to its first battle location. I had to talk to a commander of the unit and find out how long it would take them to move up.

I wrote up this little game. It had a bunch of nomograms in it, look-up tables, and it was designed so that you could calculate a day's operation of about 15 divisions on a side in about an hour, just running the calculations out. It had all kinds of calculations of nuclear weapons' effects and other kinds of effects, to calculate casualties and then from the basis of casualties to calculate movement. These were the old things that we thought were important: attrition and the movement of the forward line. We used it on a couple of exercises to match what the umpires were doing, and to make some comparisons. I got intelligence data on the locations in East Germany and Poland of all of the logistics facilities. The petroleum, oil, and lubricant, the POL facilities, the ammunition—everything we had in the intelligence structure, and I put in the eight digit coordinates of all these targets in my game. And so my game became Top Secret because all these targets were located.

But the game part itself had a lower classification, so we were able to separate out the appendix. That was the big thing I left behind at Seventh Army when I came back to the States in '64.

Back to my days with Seventh Army, 1962 to 1964, I left out a particular study that I got involved in early on. When I first got to Europe, it was October of 1962. (Matter of fact we were at sea, en route to Europe during the Cuban missile crisis, and to some degree, we were concerned as to whether we might go to war, and the ship we were on, which was a French ship, might be attacked and sunk.) An event occurred in the fall of '62. The Armalite rifle, which was then called the AR 15, which was a sporting rifle, a smaller caliber than the M14, which was the standard U.S. Army rifle, had come to the attention of some people in the Office of the Secretary of Defense.

The reputation of the Armalite was that it had tremendous casualty-producing capability even though the projectile was smaller than the .30 caliber or a 7.62 mm, which is a metric equivalent of .30 caliber.

The folks in the Office of Secretary of Defense, in Alain Enthoven's analytic group, felt that the Armalite rifle would make an admirable weapon for what was beginning to turn into a war in Vietnam. The Kennedy administration was beginning to think about sending over some advisers.

The Army was deeply concerned about the AR15. Although the weapon itself was of some interest, the Army was satisfied that the M14 was a fine rifle. The OSD people were pressuring the Army to take on the Armalite and make it the standard infantryman's weapon, so that we could buy more of them and ship them to the Vietnamese for them to use.

The Army resisted on a couple of grounds. One was bureaucratic, which was that we had an agreement with NATO to have a standard round, the 7.62, or .30 caliber round, and all the NATO nations had agreed. If you went to the smaller caliber, in the Armalite (whatever it might be called) that would violate that agreement that we had with NATO.

OSD people said the agreement was not all that important, and besides there was no demonstration that NATO ammunition could be used by the M14. So the Army was directed by OSD to do two things. One, to examine whether or not NATO ammunition could in fact be handled by the M14, and, secondly, to run a series of field operations in which they gave Armalites to infantry soldiers in different parts of the world, have them go to the field, to see how it might fare in a field exercise. The Army was directed to do this in different climates. I forget where the cold weather thing was done, but Panama was the hot weather one.

We were also directed to run a test in Germany, the only place you could get the NATO ammunition, to examine whether or not NATO ammunition could be fired by the M14. I was given this experiment to run. I was given a platoon of soldiers. At that time we had something called the Roto Plan. We rotated battle groups from stateside locations, attached them to divisions in Germany in the Seventh Army,

and they would replace a group that would come to the States, on 90-day rotations.

So I had a platoon from a Roto Plan group. We went up to Baumholder in Germany. Baumholder is an extremely unpleasant place to be, most of the time. This was in November, and the weather was pretty miserable—raw, gray clouds all the time, lots of overcast, and it was the week before Thanksgiving.

At that time, the Army was still using the known distance range thing, so we had a very boring situation for this platoon of soldiers, because only about ten or twelve of them could be on the firing line at any one time. The others were lolling around, waiting for an opportunity.

I had ammunition from the French, British, Germans, Canadians, and the U.S. I had five different countries' ammunition, all purporting to be NATO standard ammunition. The way I ran the trials, or the experiment, or demonstration—was I had armorers standing behind the line loading the clips for the M14 as we were firing—the M14—so that the shooter didn't know which ammunition he was getting.

It was a blind test, in case the shooter's behavior might change if he knew what kind of ammunition he was firing. One sidelight was when we broke open the boxes of Canadian ammunition, we discovered that it was U.S. ammunition manufactured in St. Louis but repackaged for Canadian operations, because Canadians use a plastic bandoleer. So that's how the boxes were packed, with these bandoleers. So we really were firing two batches of U.S. ammunition plus German, French, and British.

The measure of performance was to be the number of stoppages that were due to ammunition.

The soldiers fired thousands of rounds of ammunition. There's a way to set the M14 on automatic fire. So we had automatic fire as well as single, repeating rounds. Those youngsters fired more ammunition in those few days than they had ever fired in their lives, or ever would. Literally, thousands. I tried to gather data on the hit distributions as well because they were shooting at targets, and the targets were being marked, and I tried to collect some data to see whether or not the different ammunition was behaving differently.

I wasn't able to do a very good job with hit probabilities. I collected the targets afterwards and tried to do some analysis, but it was well beyond me, I mean, just the quantity of it. My crude observation was that as far as hits were concerned, there wasn't any difference.

What was significant was that there was not a single stoppage that could be connected with ammunition. There were stoppages and as soon as a soldier had a jam, or a stoppage, he immediately put his weapon down, and we had an armorer pick up the weapon, take it back of the line, and clear the jam, and determine what caused the jam. All the jams were caused by soldiers' failure to keep the weapon clean.

There was never a single stoppage from these thousands of rounds of ammunition, and of course that's the report that went in. We also took that same platoon and gave half of them AR15s and the other half had their M14s, and they went out into the field from Baumholder for two or three days. The soldiers swapped the weapons, so we evened out the behavior characteristics of individual soldiers. They ran around out in the woods, and fell down on the ground, and used the weapon—when they jumped into a firing position on the ground, you used the weapon to stop your fall.

The only problem we had was that the AR15s, which were not militarized, suffered very badly under those conditions. But that was a minor point.

The downside for the soldiers was that the week that they took the field was Thanksgiving week, so they marched out at Thanksgiving time. I was blamed—I didn't stay, by the way, for that part of it. As soon as my firing stuff was over, I went back to Stuttgart where my office was, and we wrote up the report, and shipped it in, and we clearly had demonstrated that NATO ammunition could in fact be fired through the M14.

But it made absolutely no difference, because shortly thereafter, the Army was forced into giving up the M14 and to release contracts to the Armalite Company to make a military version of the AR15, which became the M16, now the standard rifle. So we have a U.S. rifle in NATO that doesn't fire NATO ammunition anymore. That's that story.

The next big adventure was the Vietnam War. The Vietnam War was just beginning. The advisers had gone in '63, and our first adviser death was in '63. It was beginning to look like it'd be more exciting, more important than anyone expected, and I kept talking to the RAC management about sending a team in Vietnam to work with the troops.

At that time RAC was trying to build a different image for itself, and it wanted to operate at a higher level, a strategic level. They were interested in not only doing work for the Army but also doing work for the Defense Department, moving out of the old traditional roles.

I had a falling out with the management over that. In '66, a friend of mine working for a firm that had a contract with Combat Developments Command, which is the precursor to what is now TRADOC (Training and Doctrine Command). Combat Developments Command dealt principally with doctrine. Training was done by another command, the Continental Army Command.

Combat Developments Command had a contract with a small group called the Combat Operations Research Group, or CORG, which was a subdivision of a company called Technical Operations Incorporated. They were located at Fort Belvoir, where the Combat Developments Command had moved.

My friend was now running the Combat Operations Research Group. He called me up and said, "How would you like to take a team to Vietnam?" And I said, "I'd love to take a team to Vietnam." I departed RAC on friendly terms, but the reason was over this issue of not going where the troops are. I took a team of half a dozen civilians, accompanying a team of a 100 officers, to go over and do a study of mechanized and armored combat operations in Vietnam.

We got over there in January '67. The Army had sent a team over the year before to do a study of infantry operations. One of the junior analysts on that team was E.B. Vandiver. That's a very junior analyst. E.B. was working for Combat Operations Research Group when I joined them. I was senior to him because I was older; he was just a kid at the time. I found out, not too long ago, that he hated my guts because I got the team to go on the MACOV (for Mechanized and Armor Combat Operations, Vietnam) Project in '67, and he wanted that team, but he was not senior enough to be given the team, and for 20 years or 30 years, he's hated my guts (not seriously, though).

So we went over to Vietnam. We were located in a compound in Saigon, and the officers fanned out into the countryside. We had data collection forms. They were collecting information on how M48 tanks and M 113 armored personnel carriers were being used. The M 113s were being used as fighting vehicles.

They had put some weapons on them. They already had a machine gun but they added some grenade launchers. They put a lot of sandbags in the bottom to prevent mines from destroying the bottom, and they were being used as fighting vehicles.

We contacted the units using them to find out how they were using them, how effective they seemed to be. Did their experience imply anything for future, either future design, future operations, or future doctrine, for fighting that kind of a war with armored vehicles?

My civilians were not permitted to go out in the countryside. The general who commanded the study didn't particularly care for civilians and didn't care for me. I did get out a little bit. I went out with the general a couple of times into the Delta, and a few other places, but, by and large, we were stuck in the compound.

Each of my civilians were experts in particular areas. One was a logistician, another a weapons guy, and so on. I worked with a small team of officers to do the synthesis. The officer team consisted of: a brand new colonel named Donn Starry who later became a four-star, and the TRADOC commander. He also later commanded the Armor Center and the Armor School. A Lieutenant Colonel George S. Patton III, who also has a reputation, and there was another armor officer. We were the synthesis group in the study and we were in a little cubbyhole of a room, and armor officers have to smoke cigars—it's required by law—and we had a little air conditioner pumping away, but that room got to be awful foul.

But the study was exciting. We were there a few months. When I got back to the States, I was *persona non grata* with the Army because of my conflict with the general, and, to some degree, with George the Third.

The first thing I did when I came back was I was asked by the Combat Developments Command, the very people who gave me a hard time later on, to write the command position on the MACOV report. And I said, "But I wrote the MACOV report, how can I write the command position?" They said you're admirably suited to write the command position. So I had to criticize my own work. I did have a feeling that my career might have been damaged because of this personality conflict. So I had to think about what my future was going to

look like. I talked to some friends on the Army staff. They looked into it, and they said we'd be happy to have you come to work for us, if you want.

They offered me a job, but instead I went to work for a friend of mine, George Milly, who used to run the Chemical Corps Operations Research Group. He had formed a little company to do not only military analysis but also environmental stuff. I thought I'd work in a real private sector job. I went to work for him in '68. And I recruited Joanne Langston (now a senior civilian faculty member at the Defense Systems Management College). I recruited her to come to work at GEOMET, a little company out in Rockville, and the first study we did for Defense Department was a chemical study.

We wrote a series of papers that were compiled into a big source book on everything that was known about chemicals and chemical weapons. The Army (or the Defense Department) then picked it up and continued updating it.

Then I did a study on the first nonmilitary thing I think I ever did, after all these years. It was a study on the effectiveness of community health centers, a pilot program of Johnson's War on Poverty and the Office of Economic Opportunity. Joann Langston chaired the study. It was an interesting study that had some impact, subsequently, on that part of the War on Poverty program.

I did some other work during that same time on publicly provided family planning services. Did that mostly through GEOMET.

After a few years at GEOMET, I had an opportunity to join a small black-owned company in Washington called the National Institute for Community Development, located in downtown Washington. I was brought in to form a quantitative analytic group, to help them get some contracts.

They were basically a soft analytical company. They were mostly members from the Foreign Service or USAID, and they were doing a lot of touchy-feely things. We did some studies for Health, Education and Welfare. Some were on drug abuse prevention. I did more work on family planning again, some innovative stuff having to do with trying to bring young men into the family planning decision process, focusing on young men's relationships with young women.

About this time, I got interested in the master's program at University of Southern California, in Washington, in their School of Public Affairs. I did a master's in public administration and policy analysis. It was a very interesting program.

About that time, because I got interested in this policy arena, I decided that I would like to get back to a place where I might have some influence on policy. I argued to myself that you could do that better from inside the Government, and so I began scouting around for a Government job.

Well, I benefited from a strange anomaly in the Civil Service structure. I had resigned from Civil Service when I was working at Dugway, back in 1956. Because I had been a permanent civil servant out there, I had status. Twenty-three years out of Federal service, but I still had status.

A friend of mine was working in a little-known agency, the United States Metric Board. In 1975, the Congress passed a law called the Metric Conversion Act of 1975, and one unique characteristic of the Metric Conversion Act was that it denied to the Federal Government the option of converting to the metric system. The Federal Government was supposed to follow the private sector. The Metric Board was a presidentially-appointed, senatorially-approved body of 17 or 18 public citizens, chosen to represent different sectors of the society—the engineering community, the education community, labor, small business, big business. These board members were to coordinate the voluntary conversion to the metric system. And if you can imagine coordinating a voluntary process, it's kind of difficult.

The board went around the country holding hearings, listening to citizens complain or advocate conversion to the metric system. The board was supported by about 35 Civil Service staff, one of the smallest federal agencies ever to exist, but very high up on the hierarchy because we reported both to the White House and to the Congress.

It had a three-person research group and an annual contract budget of $200,000. The agency's budget was $3 million for the whole agency. I was hired as one of the three-person research group. Our job was to try to determine the impact of private sector conversion on segments of U.S. society.

In 1980 Ronald Reagan was elected President and we know, traditionally, that the incoming President cannot affect the budget that year. He has to live with the budget, the pre-

decessor budget. But Reagan's Director of OMB found the United States Metric Board was there for $3 million, and he said, "What in the devil is this?" and wiped it out, just like that.

When they close out, federal agencies are supposed to close out in the black, if at all possible. And since we were nothing but people, the idea was to try to get those people jobs. The board was desperate to get rid of people, to get them other jobs as quickly as possible. Joanne Langston—she had worked with me at GEOMET—was in Government now and she was working with the Chief of Staff of the Army. She had an office called the Study Program Management Office.

She picked me up to help develop an evaluation plan for the Army Study Program. The Army Study Program was basically the operations analysis that was going on throughout the whole Army. I started working with Joanne's office on the detail, and later was hired on directly. So that's how I got back into the Pentagon.

Joanne sent me up to Harvard for what Harvard called the senior officials in national security, eight week program. She also pushed me to apply for the National War College, which I did, and I was the Army civilian selectee in 1986 to go to the National War College at Fort McNair.

The spring of 1987 was when the Goldwater-Nickles Act was implemented, and there was a lot of reorganization of the Army staff, in particular, as well as the Air Force staff and Navy staff. When the smoke cleared, Joanne's group was no longer working for the Chief of Staff of the Army.

Her group was now working for Walt Hollis, the Deputy Under Secretary of the Army for Operations Research. I was still at National War College, graduating in June. I went over to see Walt. I had known him a little bit, through contacts when I was working for Joanne. I had an interview with him, and we were chatting about what I should do. I had become Joanne's deputy, but under this organization, there was really no requirement for a deputy. So Walt said, "What do you want to do?" and I said, "Well, I'd like to get back to doing some analysis, and if it's all the same with you, I'd like to do analysis in your group." He had a small group of analysts.

And he said, "That's good, because that's what I want you to do, too." When I graduated, I went right to work for Walt, and he was very kind to me. Most of his analysts are given systems to track for him. Part of his responsibility was in the acquisition arena, acquisition and evaluation-analysis-testing business. He's got four or five civilian analysts and two or three military analysts, and each one of them has a hardware sector to deal with.

One of them deals with armor. Another one deals with air defense. Another deals with artillery, or different sets of equipment, and it's their job to stay ahead on whatever's going on in those areas, to make sure that Walt's aware of what's going on.

With me, he said, "What is it you would like to work on?" and I said, "Well, I'm not really hardware systems-oriented. I'd like to work on some software arenas, fuzzier arenas, if that's okay with you," and he said, "Yeah, that's good."

So I got back into the chemical/biological stuff a little bit, mostly in the casualty arena. I became his representative to MORS, and I was the Army representative for ten years with MORS.

Eventually, when things like operations other than war came along, as a topic, I became his guy on operations other than war. As I said before, I've dealt a lot with the casualty stuff. I used to sit on something called the Casualty Estimation Steering Committee. The Deputy Chief of Staff of Personnel was responsible for casualty assessment in the Army, and it was exploring how casualties should be estimated.

The board itself was a collection of staff people, and they looked for things to help establish some predictions that the Army could deal with. The predictions the Army had in most of its doctrinal documents were extrapolations from World War II and Korea, and, clearly, weapons were changing and concepts of operation were changing.

A lot of the war games were doing a poor job of determining casualties. So there was interest in casualty assessment, and I spent a lot of time with them.

You asked about my involvement with the Brits. When I was working for the Seventh Army in Germany, we had visited the Army Operational Research Group, which was the British group located at West Byfleet in England, a group that had come out of World War II and was still operating, although somewhat larger, and doing broader things.

One of the people I had met was Ronnie Shephard, who was a very good analyst, got his

start in 1942 when he graduated from Cambridge, and immediately went to work in the British tank business, and he stayed in that business as an analyst, from 1942 on.

When I met him in 1963, he was pioneering some war-gaming stuff, doing table games, but using them to help think through weapons configurations and other things. Some very creative stuff. We became friends.

When I was working for Walt, I came across Ronnie again. He was now retired from British civil service and working as a consultant to Royal Ordnance, which was like a government corporation but it did private business as well. He was also doing some consulting back here in the States.

Some years earlier, Ronnie had started running an international meeting. NATO used to run a series of symposia in operations research, and NATO used to have something called the Advisory Panel on Operational Research, APOR. Back about 18 or 19 years ago, NATO pleaded lack of funds, and they closed out the Advisory Panel on Operational Research. Since they no longer ran these international symposia, Ronnie Shephard stepped in and filled that gap, and he said there's got to be an international meeting of military operations analysts. He organized his own, and, eventually, it got to be called the International Symposium on Military Operational Research. It was carried out at the Royal Military College of Science, Shrivenham, which is equivalent to our AFIT and Navy Postgraduate School, but shorter programs.

Ronnie had been teaching operations research at the Royal Military College of Science. So it was a natural to start having the symposia there. I started going to those symposia, maybe the sixth or seventh one, and I've gone to them ever since. After the tenth symposium, Ronnie was worried about what was going to happen to these symposia when he was no longer around. So he got the U.K. Ministry of Defense to agree to lend its name, and a small amount of money. He got the Ministry of Defense to establish a three-person committee to plan and continue running what we now call the ISMOR. Ministry of Defense agreed to establish a three-person committee, Ronnie Shephard, David Faddy, who was another old-time analyst, and myself, because the U.S. was the next major country involved in these symposia.

At the meetings, you'd generally have 40 percent Brits, 30 percent Americans, and some French, German, Norwegians, Swiss, Singaporeans, Turks. Most recently, we even had some folks from Eastern Europe.

When Ronnie passed away, I took over the U.S. side. With Walt Hollis's help, I manage an American mailing list of about 300 people. We have the meeting in late August. This past August, we had the 17th symposium but for the first time not at the Royal Military College of Science but rather at a conference center near Oxford.

We invite people. We send out announcements. People read papers. It's like MORS but not classified, and truly international. It's a four-day meeting. We have a half day trip to Oxford, to go to the bookstores or the pubs. And I continue to have connections with folks working in the OR business, the chemical/biological business, and in Porton in England.

Now that I'm retired, I'm doing some independent stuff. I teach a course at George Mason on the history of operations during the war. I'm trying to write some books on different things. But I'm basically lazy and I don't want to work too hard!

MR. SHELDON: How do you feel about the state of operations research as a profession today?

MR. VISCO: I've had a deep concern about the emphasis on digital models for a long time. I was quite concerned, even up to a couple of decades ago, I began getting particularly uncomfortable because we were having great trouble validating the models, and we were trying to brush off the fact that we couldn't validate them, by making all kinds of excuses for ourselves.

I went back and I looked, historically, at how we got into this situation, and wrote a paper on this. I gave a keynote address at the first Cornwallis meeting up in Nova Scotia five years ago to discuss it.

In the early days, when we first started out in military operations research, post World War II, operations research in this country, when it really began to burgeon, we began looking at ways to emulate or simulate military operations, and we began exploring computer models. I remember at ORO we had an analog computer that we were using to simulate battle—very primitive. We actually built analog computers using resistances to represent behavior of forces.

I think the first piece of work done on a digital model was in 1955, by Dick Zimmerman, who built the first combat model at ORO.

It ran on a vacuum tube machine, so it was a very small model. We wouldn't even run it on the 1103 Univac that we had. We had to go to a contractor that had a somewhat bigger vacuum tube machine.

Dick's argument was that you had to understand all the relationships that you were working with on the battlefield. You had to have closed form equations of all these interactions, and you would use the digital computer only when the number of computations got to be onerous. You couldn't do them by hand. So you had to really understand what you were doing. His paper on Monte Carlo modeling of combat, which was published by Johns Hopkins Press in 1956, won the Lanchester Prize from the Operations Research Society of America that year.

That model later became known as Carmonette. Carmonette is the words Monte Carlo turned around. That's where we got the name Carmonette from.

I think what happened was that as long as we were in the vacuum tube days, we were going to struggle with these small models. When the transistor was invented, computers got a little bit smaller. You could run them in the summertime now because you didn't have the heat loads. Programming became a little easier, and running runs, they could be done a little quicker than you could do them on the vacuum tube machine. People said, "Oh, wow, that's great. We don't have to limit ourselves to the 20 or 30 entities that Dick Zimmerman was limited to. We can now talk about lots of entities because we can write the programs faster. Even though we don't clearly understand these interrelationships, we can still go ahead and write the programs, and then go ahead."

And then a short time later, the printed circuit came along, and then, God forbid, a few years later, the chip. And I think what happened was people were seduced by the computers, and we jumped into large-scale modeling of combat, without clearly understanding all of the interactions and all the synergisms that go on the battlefield. And I think had we done what Dick had recommended back in 1955, that we proceed very cautiously until we understand all those interrelationships, we'd be in a different place.

Back to your question. I deplore what has happened with the models. I know that there's no way of turning that around now. It's impossible. I have to convince people that this may be not the right direction to have gone in.

It's unfortunate that we have gotten to the point where modeling and simulation, or M&S, are words in their own right. People don't make the distinction between models and simulation, or if there is no distinction, why do we use both words? I would like to see analysts get back to the fundamentals of military operations, and get a better feel for what really goes on in combat and out of combat, if you're trying to analyze those things. It's not a good thing that a large number of our analysts, particularly younger analysts, don't really know very much about military operations.

I'll just wind it up by saying again why I thought we had made some basic errors, early on. I felt that in using our fundamental analytic tool, which now are digital computer models, we had made some errors in the early days and were suffering the effects of that now. Because we have not done a good job of understanding combat, we can't replicate it very well on computers. That is why we have such great difficulty in validating models today. I don't believe any combat model can be properly validated.

I talked about what I thought was a major failing in military operations research, which is excessive dependency on digital models. There's no way to turn that ship around, except maybe to keep arguing that there are other kinds of analyses that need to be done. Old-fashioned war gaming may be an approach that might make some sense to revisit.

Advice to young analysts? The biggest difficulty I see, now, with young analysts, is a lack of a sense of what military life and military behavior is really all about. Even, to some extent, what military equipment is all about. We have a number of analysts coming in, coming straight out of graduate school, with no military experience. Although we've had some wars, we're not seeing any analysts coming out of those experiences.

Young analysts need to know that our job is really to see to it that young folks in the military will be sent out in harm's way—to use the Navy term. We send out young people to be at risk, to be hurt, to be killed in some cases, to defend the nation, and to carry out other missions for the nation. We need to have an ethic that says we're not doing casual kinds of analyses. We're doing analyses that affect the life and death of young people. We want them to have the best opportunity to come through

whatever military operations they have to carry out with a high probability that they're going to come home safe, with all their limbs.

Young analysts need to get more acquainted with military life. I know that's hard to do, but some organizations do a better job than others. Some of the Army analytic groups have what they call the "greening" of young analysts, where they actually try to put young analysts with soldiers, on occasion, even going out on exercises.

I'm urging young analysts to push for more of that, to get them out with the soldiers, the sailors, the airmen, and the marines, so they get to know more about what that life is all about.

I have a Latin phrase that I attach to my e-mail, which is the motto of the Royal Society in England, and it is *Nullius in Verba,* which means something like the word is nothing, or the word is nil. The interpretation of that is don't accept words that you haven't validated yourself. Make sure that you really understand what somebody has done, either experimentally, or analytically, and try to reproduce that yourself, if at all possible.

Now I realize that's impossible for lots of things, but constantly test the words of others to satisfy yourself that they are as good as possible. As a closing note, I would paraphrase Blackett, who is seen as, if not the father, certainly one of the fathers of military operations research. Blackett had a philosophy which goes something like this. When you are providing advice to military officials, senior people, recommendations, ask yourself if you were in the position of authority, responsibility, would you in fact act the way you are suggesting these people should act. Put yourself in their shoes, to test out whether or not you really feel that confident about your own work and the contribution you're making to national security.

MR. SHELDON: Your interest in studying history, where does that come from?

MR. VISCO: That's an additional piece of advice that I would give. It is important to understand military history and to read a lot of military history. That's advice that's often given to military officers, and people that are concerned with the military life. But I would also give it to military analysts: read a lot of military history. Find out the value you can get from reading history, and how it will influence your analysis.

You don't want to spend all your time on military history. Especially since humans have written military history, it's fallible in its own right. Multiple historians can look at the same operation and interpret things differently. But if you read lots of history, you'll get a flavor of that, and reading the history will also give you an appreciation of military life, and dangers, and the things that can go wrong on the battlefield, or in the air, or at sea.

My use of the military history, of operations other than war, is simply the fact that we've carried out a lot of such operations in our U.S. history, and I felt that there was something to be gained by getting a better understanding of the earlier operations and looking at differences between old operations, the first 150 years of U.S. history versus the more recent operations in the last 50 years—how have things changed, and what our military is called upon to do, in addition to fighting and defending the nation by going to war, or fighting off enemies, carrying out policy.

The fact is that military people have done lots and lots of things other than fight, and history bears that out. So looking at history, looking at the historical events, and trying to figure out a way to gain knowledge from those historical events is what I've been doing recently, and, hopefully, have a paper published one of these days.

MR. SHELDON: What piqued your interest in the Buffalo Soldiers?

MR. VISCO: Well, as part of this looking at the history of operations other than war, I got acquainted with a lot of the domestic operations that had been carried out by military forces, some not so pleasant, like strike breaking, and things like that.

It came to my attention that the soldiers on the frontier, particularly after the Civil War, there were two black cavalry regiments formed by an act of Congress, and also four infantry, black infantry, regiments of black soldiers, infantry ones, which were later folded into two regiments when they did a drawdown.

So we had two cavalry regiments and two infantry regiments of black soldiers. I discovered that, in looking at operations other than war, that these soldiers carried out a lot of things—surveying, protecting the railroad builders, protecting settlers, running miners off of Indian reservations, which was supposed to be banned, to whites doing mining, or hunting.

And the two black regiments, Ninth and Tenth Cav, were left on the frontier from about 1867, when they were formed up enough to

take the field, until about 1892, when the Indian Wars ended.

So they had a lot of experience on the frontier. They chased bandits, cattle rustlers, they supported local sheriffs. So I got vastly interested in their performance, and, by the way, they performed magnificently, had very low rates of desertion, very low rates of drunkenness, very low rates of courts-martial, much lower than the famous Seventh Cav, for example. A badly neglected part of U.S. history.

I also discovered that the regiments very rarely were ever together as regiments. They were scattered over thousands of miles of Western territory in small camps, and posts, and stations. You had company size units all over the place. They were called companies for many years. In the latter part of the 19th Century, they were called troops.

I discovered that the Ninth and Tenth Cav units had been, at various times, at 50 different forts west of the Mississippi, and I've now taken on the mission of—just to satisfy myself, to try to visit those 50 forts, and maybe write a book, or something about that. I don't think anybody's ever visited all fifty—all the forts don't exist anymore, but at least I know where they are—or where they were, and so far I've only got two of them under my belt. So I've got 48 left to do.